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Response to the comment of Foster et al. titled "Comments on Betzalel et al. "The human skin as a sub-THz receiver–Does 5G pose a danger to it or not?" [Environ. Res. 163 (2018): 208–216]"

It is with pleasure that we see that our article, "The human skin as a sub-THz receiver–Does 5G pose a danger to it or not?", has provoked a discussion as that was our intent. We also report that in violation of the norm of scientific correspondence, Foster contacted a junior member of our team for information regarding our article, rather than the designated corresponding author. We may assure him that he would have had full access had he done so. In addition, Foster et al. appear to be unaware of our previous publications on this subject (Betzalel et al., 2017; Feldman et al., 2009, 2008; Hayut et al., 2014, 2013; Safrai et al., 2014, 2012).

It is a fair question to ask if there are any possible health effects from 5G. In fact, the conclusion of our article is a call for additional research on this very point. The initial role out of 5G will only cover frequency bands up to 29 GHz, but in the future higher frequency bands will be implemented (working group IEEE P802 for WPAN standards). Consequently, a call to examine possible health effects is timely. The standards that industry shall be using for 5G are still based on the ICNIRP thermal avoidance standard established in 1998 and implicitly accept the direct absorption of CW electromagnetic energy by tissue, as the only modality of tissue/EM interaction. Our point, as clearly stated in the introduction, is that the ICNIRP thermal avoidance assumption is no longer valid. Their failure to understand the importance of non thermal effects of millimeter wave radiation reflects a basic misunderstanding of our findings.

We will not discuss their temperature simulation and estimation, as we did not make temperature calculations ourselves. However we would point out the simulation work of Shafirstein (Shafirstein and Moros, 2011), who did show more significant temperature effects than those considered by Foster, and the work of Neufeld (Neufeld and Kuster, 2018), who considered the pulsed nature of cellphone radiation at 5G frequencies and showed that by linear response theory, it will lead to significant thermal effects.

Their statement that our simulation is flawed because an isolated helix will lead to the accumulation of charge at the base and hence a distortedly large field is disingenuous. At the frequencies of our paper the absorption of the signal is such that for a sweat duct of 270 μm the impedance mismatch at the bottom of the duct is irrelevant (Hayut et al., 2013). The behavior is dominated by the natural impedance mismatch between the skin surface and the air. Consequently, our SAR distributions reported in Fig. 11 of our paper (Betzalel et al., 2018) are not, as they state, artifacts. A further point is that their knowledge of the real nature of the uniformity of the sweat duct is also misinformed. Not surprisingly, there have been some major advances in the understanding of sweat ducts since the paper of Wells and Landing some half century ago in 1968 (Wells and Landing, 1968) that they quote. They are directed to see Optical Coherence Tomography studies of the human sweat duct that shows that they truly are helical and quite regular (Tripathi et al., 2015), rather than "meandering" in irregular patterns.

The authors are correct that we did use a lower power density of $P = 1\,[\frac{V}{m}]/377[\Omega] = 0.0027\ W/m^2$. However, they ignore the message of Figs. 13 to 16 of our paper. In each figure, the SAR value is shown in dB for differing levels of ac conductivity in the duct and for the model with no embedded sweat duct (an insulating layer model with little intrinsic conductivity). The latter would coincide with the authors own model (Alekseev et al., 2008) of 2008. Intriguingly their model also produces our result of heighted absorption due to a standing wave linked to skin layer thickness at 60 GHz (Fig. 2 of (Alekseev et al., 2008)), confirming the findings of our other papers in this vein (Feldman et al., 2008; Hayut et al., 2013; Safrai et al., 2014, 2012). It is the existence of the sweat duct and its ac conductivity that proves to be significant in heighted absorption above that expected from a simple layer model. This dependence on skin morphology and its composition is the real cause for concern. As the authors of the comment are aware, the practical measurement of SAR involves a human body phantom divorced from the actualities of skin, tissue or cellphone use (Gandhi, 2019; "IEEE Recommended Practice for Determining the Peak Spatial-Average Specific Absorption Rate (SAR) in the Human Head from Wireless Communications Devices," 2013).

Finally, the authors of the comment pointed out that we did not explore biological consequences of heightened absorption. Their comment is correct. However, that was never the thrust of our article. Had the authors paid attention to the introduction they would have found numerous references to biological mechanisms capable of causing non-thermal effects. Our intention was to show that the skin morphology and its components, like the sweat duct, are further sources of interaction with high frequency RF that are presently ignored in safety consideration of 5G.

Even though the SAR standard is deeply flawed as a measure of exposure, it has become a rod by which the general scientific community gauges that same exposure. Its usefulness has been and will continue to be questioned (Panagopoulos et al., 2013). Cancerous and other damaging effects of RF radiation from cellphones are now documented in animal studies (Gong et al., 2017). One must stress that these are non-thermal effects. Other non-thermal effects are being noted in human populations (Zothansiama et al., 2017). To paraphrase the authors of this comment, dielectric nonuniformities in the skin may be useful for physiological monitoring, but they may also have significant implications for understanding hazardous effects of RF energy.

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